cloud persistently hung over the island and prevented anything like satisfactory observation, and had it not been for Mrs. Gill's pluck, failure might have been the result. It occurred both to her and her husband that the cloud was only local, and to prove the truth of this suggestion, Mrs. Gill undertook a journey along the coast, starting about midnight, over great cinders and deep rifts, to a point almost four miles south. The supposition proved true, and amid many difficulties the observatory was dismounted, and the instruments removed to the south-west of the island, to a small inlet christened Mars Bay, in memory of the expedition. But what a change from "Commodore's Cottage," as the "Garrison" residence was called. Ascension is an extinct volcano, and it is now little more than a huge mound of cinders and dust. On such floor did Mr. Gill pitch his tent, and on such a base had he to erect his delicate instruments. The discomforts attending his surroundings knocked him completely up, but with the help of Mrs. Gill and the doctor he was set on his feet again, and by the ministry and companionship of the former the encampment was made tolerable. Fortunately after all these hardships and trials and doubts as to weather, the observations at the critical time were completely successful, as were a long series of subsequent comparison observations. The captain of the island and his subordinate officers deserve the greatest credit for the assistance and support which they gave to the enthusiastic astronomer and his ever-helpful and cheerful wife. After the real work of the expedition was completed Mr. and Mrs. Gill made several excursions over the tiny island, and with the exception of an oasis on the summit of the "mountain," the island seems dreary in the extreme, and Mrs. Gill failed to find the neat square gardens and paved streets seen by Sir Wyville Thomson. Altogether, on a very unpromising subject, she has succeeded in writing a really interesting and instructive book, telling us much about the islet and its inhabitants, and still more about the circumstances under which an important piece of scientific work was done. We strongly recommend it to the perusal of our readers.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Schwendler's Testing Instructions for Telegraph Lines

In the absence of my friend Mr. Schwendler in India, perhaps I may be allowed to offer a few remarks on the notice of his book on line-testing which appeared in NATURE, vol. xix. p. 192.

It must be remembered that the book is primarily intended for the use of the officers of the Indian Telegraph Department, and that the conditions in that country differ very much from those in England. Here the overland lines are in such positions that any accident happening to them may be easily detected, but in India the lines run in many parts through countries with few inhabitants, and the distances between stations is sometimes very great. Formerly when a breakdown or fault occurred, the line-riders were sent out from the stations to find out what was the matter, and Mr. Schwendler gave a very amusing account of two of these natives riding out from the two ends of the line to find a fault, meeting in the middle, salaaming and asking one another if anything wrong had been seen. On receiving a negative reply they salaamed and rode back, but the line was none the better for it. It must be, to say the least, tedious to ride many miles over a rough country staring at a wire on an Indian sky on the chance of finding a dead snake across the wires or a bird's nest on an insulator. These difficulties suggested the very systematic line-testing now in vogue in India.

It is unfortunate that the reviewer has shown such a disrespect for mathematical formulæ. There is no doubt that the book swarms with them, but it is by means of these that Mr. Schwendler has discovered many of the facts stated in the book, some of which your contributor seems to doubt. I will, with your permission, instance two cases. He writes, "Indeed it is very doubtful whether his proof that the sensibility of the bridge method is greatest when the branch and the resistance are equal is true. At any rate in our practice we find that the more delicate the galvanometer of the bridge the more sensitive and the more accurate is our test."

In the last sentence it is not quite clear what the reviewer means by a "delicate" galvanometer. I do not know that Mr. Schwendler says that the galvanometer should not be delicate, but he does say that its resistance should bear a certain relation to the resistance measured. For a high resistance a galvanometer of high resistance should be used, and for a low resistance one of a proportionately low resistance. But the alteration of the resistance should not be made by using only a portion of the coils. This is fully explained on p. 22, where it is shown that the coils should be connected either consecutively or parallel in order to increase or diminish the resistance of the instrument. Thus all the convolutions of the galvanometer are used, but as the resistance is diminished in the second case a larger flow of electricity takes place and a greater deflection is produced. The results of these theoretical considerations are so readily tested by experiment that it is surprising that the author of the notice should have thrown doubt on their accuracy. I have therefore thought that it might be useful to make some measurements which have fully confirmed the theory. I will not trouble you with all the experimental numbers of about 160 measurements, which would be as "appalling" to the readers of NATURE as Mr. Schwendler's formulæ are to the writer of the review. A reflecting galvanometer with two coils was used. Connected consecutively their resistance was 5590 units and parallel 1405. The results would have been more striking if the coils had been of unequal resistance so that the parallel resistance would have been less, or if the two halves of the bobbins could have been connected parallel, which would have reduced the resistance to about 700 units. The following table will show the results obtained on measuring three resistances with varying branch resistances, and with the two arrangements of the galvanometer the deflections of which were noted when a certain alteration of the comparison coil was made:—

Resistance tested. Units.	No. of cells used.	Resistance of branches. Units.	Alteration of comparison coil. Units.	Deflection of spot of light with galvanometer coils: Consecutive.	Parallel.
4740	20	10	20	14	15
—	—	100	—	91	95
—	—	1000	—	192	190
760	20	10	4	23	39
—	—	100	—	140	234
—	—	1000	—	333	386
90	1	10	2	47	93
—	—	100	—	77	148
—	—	1000	—	77	121

These numbers show that the branches should approximate to the resistance measured and also that the galvanometer resistance should be smaller when a small resistance is measured. Calculation shows that the most advantageous resistances of the galvanometer in the three cases would be 2870, 880, and 95 respectively.

There is another point with regard to testing with Wheatstone's Bridge, which is not noticed in the review, but to which I may be allowed to direct attention;—that is, the position of the galvanometer. It is not indifferent in which diagonal of the bridge the battery and galvanometer are placed when the branches are unequal. In such a case the method is much more delicate when the galvanometer is placed in the diagonal joining the junction of the two largest resistances with the junction of the two smallest. As, I believe, we have in this laboratory the only Wheatstone's bridge yet constructed after Mr. Schwendler's design, by which the position of the galvanometer and battery can be altered by the shifting of four plugs, I have made a few tests which will show the advantage of this arrangement.

The diagonal joining the junction of the branches with the junction of the comparison coil and the resistance measured is called *m n*; the other diagonal being *p q*.

Resistance measured. Units.	No. of cells used.	Resistance of branches. Units.	Alteration of comparison coil. Units.	Deflection of galvanometer in diagonal *m n*	Deflection of galvanometer in diagonal *p q*
		$\frac{b}{a}$			
90	1	$\frac{100}{100}$	2	148	147
—	—	$\frac{100}{1000}$	20	53	122
—	—	$\frac{10}{1000}$	200	7½	34½

It will thus be seen that when the branch resistances are equal it is indifferent in which diagonals the galvanometer and battery are placed; but this is not the case when branch *a* is greater than branch *b*. It is hardly necessary to observe that in a practical test more than one cell would be used when the branches are unequal, in order to obtain much larger deflections, and more accurate measurements. HERBERT MCLEOD

Royal Indian Engineering College, Cooper's Hill, January 6

The Unseen Universe—Paradoxical Philosophy

THE principle of continuity forbids us to imagine that the collocation called the atom has existed as it is from all eternity. This the authors of the "Unseen Universe" have insisted upon, and I need not go further than their title-page to remind Mr. Hallowes that in like manner they do not contemplate a future eternal existence for the atom.

But this principle cannot tell us what was the exact nature of the thinkable antecedent of the present universe, nor can it tell us the exact nature of that state which will follow the disappearance of the present system. There are, however *strong scientific analogies* which lead us to believe that the thinkable antecedent of the present system was a spiritual unseen, which not only developed but which now sustains the present order.

Is it therefore necessary that I myself should in like manner help to sustain some inferior universe? I repudiate any such obligation. I am not fit for it.

Because a little boy has a father, is it logically essential that he should likewise have a son?

HERMANN STOFFKRAFT

Schloss Ehrenberg, Baden, January 11

Molecular Vibrations

IN NATURE, vol. xix. p. 158, col. 2, is the following:—

"It has been suggested that the same molecule may be capable of vibrating in different ways, and thus of yielding different spectra, *just as a bell may give out different notes when struck in different ways*." It is well to note that the bell as a whole gives but one sound, and the other sounds are not true harmonics, but come from parts of the bell, either before the whole is in vibration or from parts badly amalgamated, flaws in the metal, air-bubbles in pouring into the mould, lack of homogeneity, inequalities in the mould, &c.

The noises in a belfry are most discordant, whereas harmonics form a succession of consonances—octave, fifth, fourth, major and minor thirds, seventh and treble octave.

WM. CHAPPELL

The Electric Light

WHILE so many experiments are being made on lighting by the incandescence of infusible materials produced by electric currents, it is well to point out that Dr. Draper, as early as 1844, used a strip of platinum so heated to determine the facts that all solid substances become incandescent at 977° F., that light increases in refrangibility and intensity, and that the order of the colours emitted followed the true prismatic order as the temperature increases.

Dr. Draper says: "Among writers on optics it has been a desideratum to obtain an artificial light of standard brilliancy. The preceding experiments furnish an easy means of supplying that want, and give us what might be termed a 'unit lamp.' A surface of platinum of standard dimensions raised to a standard temperature by a voltaic current will always emit a constant light. A strip of that metal one inch long and $\frac{1}{20}$th of an inch wide, connected with a lever by which its expansion might be measured, would yield at 2,000° a light suitable for most purposes. Moreover, it would be very easy to form from it a photometer by screening portions of the shining surface. An ingenious artist would have very little difficulty, by taking advantage of the movements of the lever, in making a self-acting apparatus in which the platinum should be maintained at a uniform temperature, notwithstanding any change taking place in the voltaic current." (*Vide* Draper's "Scientific Memoirs," p. 45.)

Wimbledon, January 11 W. H. PREECE

Force and Energy[1]

III.

IN consequence of energy not being a directed quantity we come at once upon an important distinction between transference of energy and transference of momentum. There may be a large force exerted, *i.e.*, a large amount of momentum rapidly transferred, without there being any accompanying transference of energy. In the distance V on the two sides of a given section of the stressed material through which the two opposite streams are flowing, there is lodged a certain amount of motion which is the same in the one portion on the one side of the section as in that on the other side. The momentum and the energy lodged in each portion are simply different functions of one and the same motion. In unit time the whole of the motion in the portion on the one side of the section is transferred into the portion on the other side, and *vice versâ*. The resulting quantitative transference of the one function of the motion is double what would take place if only one, instead of two, opposite streams were flowing through the section, the reason being that this function is a directed quantity. The resulting quantitative flow of the other function of the motion is zero, because it is a function which has no direction. The rate of transference of momentum, or the force, is in this case eE, the sign being given by the sign of e. Suppose, now, one only of these streams of motion to be flowing past the section, the rate of transference of momentum being $\frac{1}{2}eE$, where e is the geometrical ratio of extension, or the strain. The rate of transference of energy remains to be calculated. The material may be either at rest or in motion. In fact whether it is to be considered at rest, or at what velocity it is to be considered moving, depends altogether upon the set of bodies relatively to which the motion is to be measured. Its relative velocity may also be either uniform or variable. The relative velocity of the centre of inertia of the material lying between two given sections will be uniform if the whole of the motion measured in any quantitative way flowing in through one of these sections is equal to that simultaneously flowing out at the other.

Suppose that before the force begins to act there is a uniform velocity, v_0, throughout a given length. As soon as there is a uniform force, $\frac{1}{2}eE$, throughout this whole length, the flow being only in one direction, one half the particles will have at any instant the velocity, v_0, while the other half has the velocity $(v_0 + v)$, where $v = e\sqrt{\frac{E}{\mu}}$.

$V = \sqrt{\frac{E}{\mu}}$ being the velocity of stream-flow; there is in the length V lodged an amount of momentum $(V\mu v_0 + \frac{1}{2} V\mu v)$ for unit section throughout that length. Of this amount $\frac{1}{2} V\mu v = \frac{1}{2} eE$ is transmitted forwards per unit of time. The mean velocity of the material is also $(v_0 + \frac{1}{2}v)$.

[1] Continued from p. 219